as usual either cannot or will not reply straightforwardly, but talks about side issues. Perhaps he will be good enough to tell us what the main issue is.—I am, etc.,

Friern Road, S.E., Nov. 11th. A. MACLEAN.

LAPAROTOMY FOR PERFORATION IN TYPHOID FEVER.

SIR,—I have read with great interest Mr. Francis Heuston's account of his brilliant and successful operation for perforated intestinal ulceration in typhoid fever; and I heartily congratulate my old school-fellow on his treatment and its results, and Dr. Wallace Beatty on his prompt diagnosis.

I fear, however, that Mr. Heuston is in error in believing that it is only necessary for physicians to be aware of the success to be expected to submit their cases more frequently to operation. The true reason that we do not resort to surgery more often in these cases is the difficulty, or rather the impossibility, of determining in most instances whether perforation has taken place or not. When it occurs the patient has generally been in a state of profound prostration for some days—prostration attributable to prolonged pyrexia and septic absorption, and perhaps aggravated by intestinal hæmorrhage, and its onset is not marked by any special pain or exacerbation of the symptoms. Consequently it seldom happens that the physician is able to diagnose the existence of perforation with sufficient confidence to make him feel justified in advising laparotomy, at any rate until the existence of peritonitis becomes fairly obvious, when the time for successful operation has probably gone by.

For some years I have been on the look-out for perforation in typhoid fever, with a view to operation, but only once has the diagnosis been sufficiently clear to make me feel justified in resorting to it. The perforation was easily found and secured, but death took place probably as soon as it would have done if no operation had been performed. In this case the diagnosis was fairly clear at the time the operation was resolved on, though probably, to be of any use, it ought to have been performed several hours before. The following case shows how a fatal perforation may fail to make itself apparent, even when we are anxiously watching for indications of its existence.

A strong man, aged 39, was admitted to one of my wards in the Liverpool Royal Infirmary on October 30th, having been ill with typhoid fever about three weeks. He was very prostrate, had subsultus tendinum, temperature about 103°, and pulse about 120. He had copious hæmorrhage from the bowels on November 2nd, 3rd and 4th. He gradually got weaker, and died on November 5th. I carefully considered the possibility of perforation having taken place, but decided against it on the ground that there was no special abdominal distension, the liver dulness was normal, there was no particular abdominal pain, and the hæmorrhage seemed sufficient to account for the progressive weakness. Nevertheless the necropsy revealed the existence of a small perforation about 18 inches above the ileo-cæcal valve, which had led to fæcal extravasation and extensive peritonitis. The hæmorrhage had apparently come from another ulcerated patch close to the valve.

I mention these cases to illustrate my contention that a favourable result from operation can only be looked for in cases where perforation can be recognised as soon as it has occurred. These will be cases which are running a comparatively mild course, or cases like Mr. Heuston's, in which perforation takes place after the patient seems to have entered on the stage of convalescence.—I am, etc.

THOS. R. BRADSHAW, M.D.,
Physician to the Liverpool Royal Infirmary.

Liverpool, Nov. 16th.

HUMAN AND BOVINE TUBERCULOSIS.

SIR,—In regard to the question raised by Professor Koch as to the communicability of bovine tuberculosis to human beings, there does appear to be some room for doubt as to whether intestinal tuberculosis is, after all, brought about by milk from tuberculous cattle. It appears that the amount of the intestinal tuberculosis that is met with may be only proportionate to the amount of milk infected, not by cattle, but by other human beings.

In the more vigorous investigation into the subject now brought about by Dr. Koch the following lines of inquiry, pursued by medical practitioners particularly, and in such ways as possible by medical officers of health also, would yield information, whether positive or negative, which would be of value, as to cases of intestinal tuberculosis and of infantile tuberculosis in all its forms:

1. Are these cases situated in houses where there are cases of phthisis with expectoration?
2. Are these cases situated in houses which have been infected recently by phthisis?
3. Are they in houses which are dark, dirty, and dusty?

—I am, etc., EDMUND M. SMITH, M.D., D.P.H.,
York, Oct. 24th. Medical Officer of Health.

THE TREATMENT OF PHTHISIS BY MEANS OF HIGH FREQUENCY ELECTRICAL CURRENTS, ETC.

SIR,—I am obliged by Mr. Chisholm Williams's courteous reply to my query regarding the enormous increase in chest girth which took place in one of his patients treated by the above method. Mr. Williams would probably be the first to admit that such an increase must be altogether exceptional. It would rarely, indeed, be desirable. The fact is that in all cases of phthisis sufficiently far advanced to produce dyspnœa the lungs are expanded as much as it is desirable that they should be. In chronic dyspnœa, as I can testify from a careful examination of a large number of cases, the inspiratory muscles act more powerfully than the expiratory, so as to maintain as far as possible, or actually to increase, the mean capacity of the chest. So potent is this expanding force that it may lead to a considerable degree of emphysema in the neighbourhood of the tuberculous areas. It is well to bear in mind, then, that whenever there is dyspnœa, or a tendency to it, as there is in all cases of chronic lung and heart disease, there is no occasion to adopt any special means to increase pulmonary capacity, for the dyspnœa will itself induce all the expansion that is desirable; expansion beyond this can only take place by the induction of emphysema.—I am, etc.,

Wimpole Street, W., Nov. 2nd. HARRY CAMPBELL.

THE PLAGUE.

SIR,—It seems expedient, in view of present knowledge, to admit that recrudescence of plague in a city, with accompanying affection of the rats, means that the disease is probably present as an epizoötic, and that its further spread is independent either of the sanitary condition of the locality where the sporadic human cases have happened to occur, or of the incubation period of plague in the human subject. "Contacts" now mean not those who have been exposed to human plague, but those who may become exposed to rat infection.

If every affected rat has been destroyed the disease will disappear; if not, its future course will probably follow the slow history of all such outbreaks: an apparent cessation for weeks or months, the rat infection spreading meanwhile, then a sporadic human case or cases in some fresh locality where opportunity occurs for rat infection of the occupants of defective premises, and so on.

Destruction of rats as a method of preparation against invasion is obviously indicated in every district, but it is practically impossible to disturb the public apathy until invasion occurs, when it is too late.

Dr. Wynter Blyth suggests a crusade by sanitary authorities directed towards the rectification of defects in basements of premises permitting access of rats. Such a crusade is highly necessary in the case not only of grain warehouses and riverside premises, but of private dwellings, and, setting plague aside, could result in nothing but good in districts adopting the suggestion.—I am, etc.,

D. S. DAVIES, M.D.,
Bristol, Nov. 11th. Medical Officer of Health.

PRELIMINARY GENERAL EDUCATION.

SIR,—With the general tenour of the paper on preliminary education of medical students[1] most of us I think will agree. It is when we come to think out the details that difficulties occur. The title of the paper offers a difficulty at once in the term "medical students." Indeed, the paper throughout seems to place the education of boys destined to become medical men on a separate footing to that of other professions. Already at the larger schools we have the classic side, and the modern side, and the army class; are we to add to this the medical class with other possible developments in the future, cutting up the scheme of general education into

[1] BRITISH MEDICAL JOURNAL, November 2nd, p. 1327.

smaller special classes? It would, I think, be a most unfortunate move, a move backwards instead of forwards. At what age is a boy to be expected to make up his mind that he has a calling to medicine or surgery? Not, I think, at 15 or even 16 years, for there is nothing in our profession proper that appeals to so young a child. Yet a year is no long time to devote to any specialised form of education having a distinct examination in view. But here difficulties do not end, for the London University has separately decided on its own standard for the matriculation examination, and thus from the first we should have two classes of boys preparing for different standards of medical examination.

The day, I believe, has gone by when lads of 14, 15, or 16 were asked to read Celsus or Galen and the like. When that senseless custom was abolished a most important point was gained; for the future medical man was admitted into the forms where he mixed with those he had to mix with when grown up, those boys who are destined to become lawyers, clergymen, sailors, and soldiers. This comradeship and feeling of equality—yes, and perhaps superiority—has done nothing but good, and the feeling of competition amongst the boys in such classes, more numerous than can be the case if boys destined for the medical profession are to be a class of themselves, has been most beneficial on both sides. Therefore I say again to have a specialised medical boy examination is a retrograde step.

What then is to be proposed? It is nowhere suggested that the medical man is to be made out of top boys of their class only, all we contend for is that he is to be equal to the average, and that the average is to be equal to the general knowledge a boy in the upper fourth or fifth class can reasonably be expected to have.

We may, I think, fairly turn to the legal profession and see if we cannot assimilate our needs with theirs. Could this be accomplished a most important and far-reaching step would be gained, for a very large percentage of all boys at school are being trained for these two professions, and thus a very important standard of middle-class education would establish itself—a want strongly felt. Were this successful it would be likely that other professions would bring their examinations into conformity with this, which would be the natural one, being the average knowledge of the mass of boys of the given age.

There would remain, therefore, for settlement the standard, and this must assuredly be done by educationalists with broad minds in the best sense. The particular point I have in my mind is not to say a boy must be able to read Cæsar, or must be able to read Horace or Cicero, or taking Greek, Xenophon, Herodotus, or Greek Testament or Homer, or any one given author. In other words, not that the examiner should decide, but the boy, that is to say, his teachers. This, which at first sight may seem trivial, is not so, for it affects the age at which a boy settles to go in for one particular profession; and if the examiners settle the author, leaving no choice, it brings us back to the medical class again.

Much remains to be said on other subjects of the school curriculum, but enough has been said to illustrate the point I desire to bring forward.

I quite agree as to the multiplication of examining centres within reasonable limits, and think it would do much to facilitate boys entering professions, and Dr. Gordon's conclusions I quite endorse, but I do very greatly desire to place our education on a broader basis, and keep all boys destined for professions competing with each other as long as possible.—I am, etc.,

J. Delpratt Harris,
Exeter School Board Manager and Voluntary School Manager.

November 4th.

THE MORTALITY IN THE BOER CONCENTRATION CAMPS.

Sir,—I am in full agreement with your contributor[1] in his general conclusions on this subject, and am glad that you have spoken out clearly upon the need for strong and radical measures.

The statistical question—how to deal with these high mortality-rates—has an interest of its own. A low mortality-rate, which influences but little the numbers of the population, may be multiplied or reduced to apply it to differing periods of time with very little error. The Registrar-General does this when he reports a weekly death-rate in terms of an annual one.

But when we have to deal with a high mortality, the error of so treating it becomes apparent. Thus, in the death-rate of 1 in 9—that of the white children in the camps for the period of four months—your contributor states that "if this death-rate be continued for a year, 1 out of every 3 will have died." Is this really so? Now the population will either have diminished during the year, or else it will be maintained by additions from without so as to counterbalance the loss. If the former, the rate operating on a decreasing population will cause the loss of, not 3, but 2.68 units out of the 9. When, however, the death-rate is estimated in the usual manner as the number of deaths divided by the number of the population in the middle of the year, this would bring out 1 in 2.82, or 355 per 1,000 as the annual mortality. Nevertheless, "1 out of 3" will not really have died, since 9 units were concerned and only 2.68 died. If, on the other hand, the population be maintained at 9 units throughout by the addition of units from the outside, 3 units will die, and the death-rate estimated by the usual method will be $\frac{3}{9}$, or 333 per 1,000. Nevertheless, 1 out of 3 will not really have died, since 12 units have been concerned—namely, 9 *plus* 3 added from the outside.

Thus, either way it is clear that a simple multiplication of the death-rate to correspond to the longer period of time does not give an absolutely true result. The error or discrepancy is the consequence of dealing with a high and altogether abnormal death-rate, which greatly alters the area of operation (population), and makes this too unstable to allow of the usual estimation, from a shorter to a longer period.

I still think, therefore, that it is better in the case of a very excessive death-rate to quote only the rate for the period under review, and that if it be multiplied in the usual way to form an annual rate, the result is to exaggerate it.—I am, etc.,

Finsbury Square, E.C., Nov. 16th. R. Hingston Fox.

INOCULATION AND THE INCUBATION STAGE OF PLAGUE.

Sir,—I had not intended to intrude further on your space, or to prolong the discussion on the above subject, but as Colonel King appears to state that Mr. Haffkine does not share my belief in the power of his vaccine to abort plague in the incubation state of the disease, I desire to state that Mr. Haffkine saw and approved of my paper on Inoculation and the Incubation Stage of Plague, published in the British Medical Journal of September 14th, when it was in manuscript.

He also has throughout given it as his opinion—surprising though it may appear—that the plague vaccine does produce abortion of the disease in the incubation stage, and that it is harmless even to those who may be inoculated with the initial plague fever on them. This opinion he stated publicly in his evidence before the Indian Plague Commission, and in reports submitted to Government from time to time. As Colonel King may not have these beside him, however, I would refer him to A Discourse on Preventive Inoculation, delivered by Mr. Haffkine at the Royal Society, London, on June 8th, 1899.[1] Perhaps the following quotation from this discourse may help Colonel King to understand that it is not necessary in order to arrive at a just conclusion in this matter to have populations "experimentally infected." In the analysis of the Byculla Gaol experiment Mr. Haffkine says:

> Did, however, this [the indisposition, pain, and fever caused by the inoculation] make them [the prisoners] more susceptible to the disease than were their non-inoculated fellow inmates? It is certain that the table testified unmistakably to an opposite result. Further, the incubation period in plague appears to be on the average five days, extending, however, not infrequently to ten days. Of the 12 prisoners in the uninoculated group who developed plague during the next few days after the date of inoculation, a large proportion, if not all, must have been already incubating the disease on that day. Seeing the perfect similarity of conditions under which the inoculated and the uninoculated were living, it could safely be inferred that a similar group of individuals incubating plague was present among the inoculated also at the time when the inoculation was performed on them. The inoculation, however, did not aggravate their

[1] British Medical Journal, November 16th, p. 1507.

[1] British Medical Journal, 1899, vol. ii, p. 11.